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Misunderstanding the "Nature" of Co-Management: A Geography of Regulatory Science and Indigenous Knowledges (IK)

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Abstract Governments, NGOs, and natural scientists have increased research and policy-making collaborations with Indigenous peoples for governing natural resources, including official co-management regimes. However, there is continuing dissatisfaction with such collaborations, and calls for better communication and mutual learning to create more "adaptive" co-management regimes. This, however, requires that both Western and Indigenous knowledge systems be equal participants in the "co-production" of regulatory data. In this article, I examine the power dynamics of one co-management regulatory regime, conducting a multi-sited ethnography of the practices of researching and managing one transnational migratory species, greater white-fronted geese (*Anser albifrons frontalis*), who nest where Koyukon Athabascans in Alaska, USA, practice subsistence. Analyzing the ethnographic data through the literatures of critical geography, science studies and Indigenous Studies, I describe how the practice of researching for co-management can produce conflict. "Scaling" the data for the co-management regime can marginalize Indigenous understandings of human–environment relations. While Enlightenment-based practices in wildlife biology avoid "anthropomorphism," Indigenous Studies describes identities that operate through non-modern, deeply imbricated human–nonhuman identities that do not separate "nature" and "society" in making knowledge. Thus, misunderstanding the "nature" of their collaborations causes biologists and managers to measure and research the system in ways that erase how subsistence-based Indigenous groups already "manage" wildlife: by living through their ethical commitments to their fellow beings. At the end of the article, I discuss how managers might learn from these ontological and epistemological differences to better "co-produce" data for co-management.

Keywords Indigenous knowledges · Power · Co-management · Co-production · Scaling

Introduction

The quickest way to travel to the Koyukuk River Valley, Alaska (Fig. 1), is to hop a Piper Navajo or Caravan to fly across the road-less boreal forest. About a 2 h journey due northwest by air from the city of Fairbanks; it would be longer by snow machines or dog mushing, but people have also walked across that country, a trek that took seasons and years (Huntington and Elliott 2002). Just because this landscape remains isolated from the highway system does not mean it is a terra incognita; on the contrary, it is a highly surveilled place. This is a landscape known by the Athabascans who have lived off this land for thousands of years, demonstrated through the intricate trail and river systems where they historically and currently travel (Fig. 1). And it is now also monitored by wildlife biologists and ecologists[1] of state and federal agencies of Alaska and the United States, demonstrated through the current land management boundaries.

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[1] Although many refuge employees are trained in both biology and ecology, regulatory agencies hire "wildlife biologists." For the sake of brevity and to reflect the actual job title, from here onward I will refer to "biologists," even though their knowledges might include those derived from extensive studies in ecology.

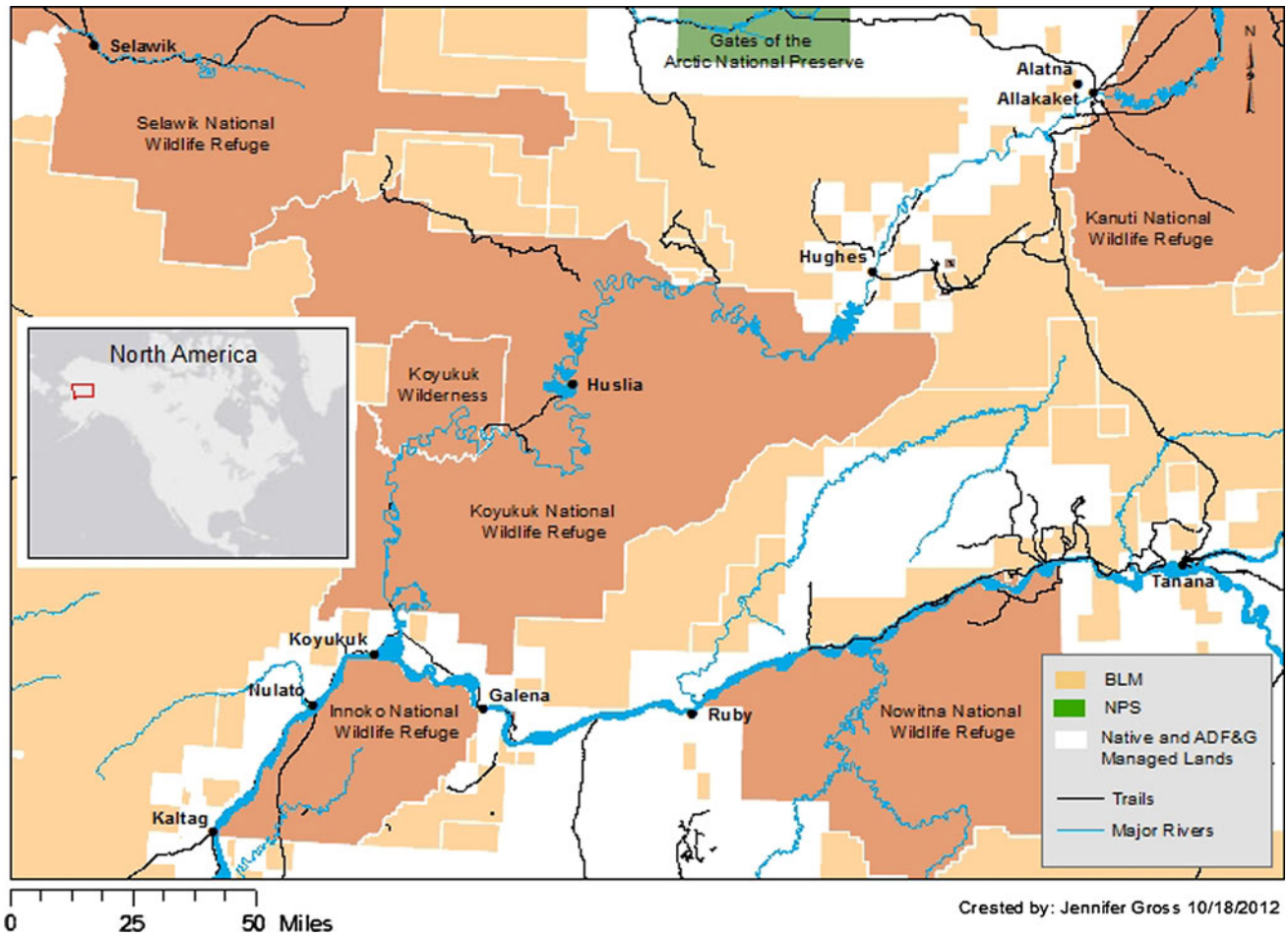

Fig. 1 Location of Ethnographic Study Sites: the Koyukuk and Innoko National Wildlife Refuges (map by Jennifer Gross)

As indicated in the map, two systems of knowledge overlap to manage this ecosystem. Athabascan peoples have lived in the Koyukuk River Valley for thousands of years, indicated by the map's trails and rivers, where they would make their passage through the seasons and manage these resources through their subsistence activities.[2] Russian exploration began in the eighteenth century, eventually colonizing just parts of the Koyukuk. It was not until 1867 that the United States purchased the Territory of Alaska from Russia, and commenced its exploration and settlement (Dall 1870). The map of Alaska eventually accrued these new land ownership boundaries—but the ways they overlapped with Native geographies had not been visible to settlers.

For as recounted by postcolonial scholarship, including fields like science studies,[3] critical geography, and Indigenous Studies, the social and natural sciences from the age of colonialism through the 1980s attributed little validity or utility to what has eventually been called "Indigenous Knowledges" (IK) or "Traditional Ecological Knowledges" (TEK) (Tuhiwai Smith 1999; Berkes 1999). Many conceived of Indigenous "culture" but few ventured claims that these cultures produced systems of knowledge equally valid to Western Sciences. Indigenous peoples were thought to tell "myths" not "facts," and sometimes these "other" knowledge systems were explicitly undermined for the purpose of Western empire building (Mitchell 2002; James 2001; Harding 1998; Deloria 1997; Scott 1996). Alaska Natives were not understood to thereby "manage" the landscape when land management boundaries became drawn on the map of Alaska.

However, in the last few decades, research has shown that "local" peoples understand both current conditions as well as changes in the local ecosystem by virtue of their spatial and temporal purview, by their ties to the landscape that come from their subsistence-based cultures and economies. Around the world, scientists of various agencies and disciplines have forged official collaborations with

[2] These are not all the trails that locals use, but indicative of the pattern of overland travel and knowledge of the landscape.

[3] I use the umbrella term "science studies" to refer to a broad literature cross-cutting the social sciences; see the review by Powell (2007).

Indigenous peoples: to describe local biota, local ecological processes, or the local effects of climate change (Fraser et al. 2006; ACIA 2004; Krupnik and Jolly 2002; Miraglia 1998). A rising discourse of IK has argued its validity in both research and governance (Laurie et al. 2005; Escobar 2001). Today both Natives and scientists are understood as legitimate witnesses of environmental phenomena: the initiatives promoted by institutions such as the UN and World Bank now require that projects recognize and integrate "local" concerns in their planning stages (UN 2008; Agrawal 1995; Wilmer 1993).

Some of these shifts in power relationships and research practice are reflected in the rise of partnerships or protocols that explicitly follow "community-based" or "collaborative" management ideals (Tole 2010; Pinkerton et al. 2008; Plummer and Armitage 2007; Coombes et al. 2011; Keough and Blana 2006; McCarthy 2006). Official "co-management" regulatory structures proliferate, whereby Indigenous peoples are accorded equal votes in making decisions about managing the species upon which they must rely (Jones et al. 2010; Meek 2010; Notzke 1995; Pinkerton 1992, 1989).

However, also increasing are dissatisfactions with what kinds of outcomes co-management achieves. For example, Indigenous peoples and many social science scholars often express disappointment in co-management or other community-based collaborations because of the dominance of "Western" sciences over Indigenous ways of knowing (Goldman 2007; Nadasdy 2003; Howitt 2001). Others claim that Indigenous peoples' knowledge systems are being over-romanticized and co-opted for decision-making (Wohling 2009; Gilchrist et al. 2005). At the meetings themselves, sometimes co-management devolves into outright conflict.

For example, at a meeting of the Alaska Migratory Bird Co-Management Council (AMBCC), a Native AMBCC representative interrupted a staff report on harvest surveys to argue about the role of Refuge Information Technicians (RITs). RITs are employees of the United States Fish and Wildlife Service (USFWS); the RIT program is celebrated as one of many ways that the USFWS collaborates with Native American tribes, providing the Native community with job opportunities as well as opportunities for tribes to better communicate within the regulatory regime (Yung 2000). The non-native staff member at the AMBCC meeting therefore reported that RIT-administered harvest surveys were "better" ways for managers to trust the data coming from hunters—but the Native AMBCC representative hastily remarked into the microphone, these RITs were not "our people," despite all RITs being Native community members.[4] As this AMBCC member continued his critique, the Chair of AMBCC, one of his fellow Native representatives, reached over and turned off the microphone. The Chair asked him to stay "on point" during the agency report on the harvest surveys—it was not the usual protocol to raise protest during agency reports. Infuriated, the member paused, pushed his chair back and raised his hand.

"Yes?"

The AMBCC representative leaned forward again, pressing into his microphone briefly. "Mr. Chair, I thought I was on the point." His outburst prompted a discussion wherein this member insisted, "RITs do not represent the communities!" Conflict, not only between Natives and non-Natives, but across the constituency of Native stakeholders, has led many scholars to emphasize the need for better communication and generating trust within co-management and other collaborative management regimes (deVos and vanTatenhove 2011; Pinkerton 1992).

More recently scholars argue an ecological need for this mutual learning, to create more "adaptive" co-management regimes (Armitage et al. 2011, 2009; Berkes 2009; Plummer and Armitage 2007; Adger et al. 2006; Folke et al. 2005). Work from the ecological sciences on complex systems reveals how nonlinear ecosystem dynamics can produce tremendous "surprise," producing uncertainties that are especially difficult for "command and control" management approaches (Plummer and Armitage 2007; Scheffer and Carpenter 2003; Holling and Meffe 1996). These scholars not only argue the need to devolve governing processes to local levels or otherwise "co-manage" resources with local peoples, but they also argue that these management systems should be "resilient" and "adaptive." That is, these "social–ecological systems" can adapt to changes by fostering linkages between institutions and communities that allow for the flow of new information, allowing for decision-makers within the system to learn (Armitage et al. 2009; Berkes 2009; Adger et al. 2006; Folke et al. 2005; Olsson et al. 2004). However, as demonstrated above, translating this philosophy into everyday practice, like during AMBCC meetings, seems less attainable.

Co-management scholars also argue that IK has an important role in learning about the system. Armitage et al. (2011) review the successes of a few "adaptive co-management" regimes through which regulatory knowledge is "co-produced." By "co-production" they mean a management regime "of evolving institutional networks and linkages (vertically and horizontally) through which understandings are negotiated and values made explicit" (Armitage et al. 2011, 1003). Although they suggest "negotiation" is necessary, they state that understanding

[4] Data from this exchange comes from author fieldnotes as well as from transcripts of AMBCC meetings.

power relations between Indigenous and non-Indigenous peoples was not their project (Armitage et al. 2011, 1997). Indeed, this literature on adaptive management and adaptive co-management largely avoids questions of power relationships in ways that have been central to the concerns of the postcolonial scholarship cited above, concerns central to Indigenous Studies. Some acknowledge power dynamics insofar that managers should be aware how differences in culture and class impacts the sharing of knowledge (e.g., Armitage et al. 2009). These scholars do not examine the power dynamics that constitute data itself.

Significantly, what is at stake in "co-production" is the power to determine what is knowledge, what is "good information" for the regulatory regime—yet this gets determined long before that information makes it to the co-management forum for "negotiation." How is it that Western Sciences and Indigenous Knowledge systems might observe the same phenomena, but find disagreement in management? In this article, I show that the outburst from the AMBCC member recounted above demonstrates differences in epistemology, as well as ontology: the categories humans devise in conceptually organizing the world (ontology) that affect how humans attempt to understand that world (epistemology). These differences produce misunderstandings between Western Sciences and Indigenous Knowledge systems.

The aim of my study was to understand how power dynamics in a cross-cultural context affect the making of regulatory knowledge. I began with the assumption that IK existed and was valid, and that the aim of "good governance" was to more effectively include IK into decision-making. This led me to use methods of ethnography and participant-observation in generating my empirical data, but also to engage scholarship on the politics of scientific practice as one framework for understanding this empirical data. My project therefore attempts to bridge the environmental management literature on social–ecological resilience with scholarship in the critical tradition: from indigenous studies, critical geography, and science studies.

The critical tradition that comes from indigenous activism and intellectual traditions claim that research itself is still a colonial endeavor, denying Indigenous peoples from "speaking for themselves" (Cook-Lynn 2001; see also James 2001; Tuhiwai Smith 1999). The literature from Indigenous Studies continues to highlight the politics implicated through the very practice of science, and today fields like anthropology and geography now confront the "colonial" disciplinary practices that still emerge when engaging with Indigenous peoples (Watson and Till 2009; Louis 2007; Johnson et al. 2007; Shaw et al. 2006; Medicine 2001; James 2001; Salmón 2000; Cajete 2000; Biolsi and Zimmerman 1997; Godlewska and Smith 1994; Deloria 1969). These scholars also argue that Native knowledges or IK are not only germane, but also significantly different from Western sciences. These differences are ignored through attempts at "integrating" IK and Western sciences, taking that knowledge out of its "context" (Nadasdy 2003, 1999; Agrawal 1995).

Furthermore, the literature from Indigenous Studies argue that sciences are "Western"—that is, they have a role in the history of empire building, used in efforts to disenfranchise Indigenous peoples (Cook-Lynn 2001; James 2001; Tuhiwai Smith 1999; Deloria 1969, 1997; Wilmer 1993). However, they argue, this did not just happen in the past; the politics of science continues to happen because science produces politics. In one example, geographer Richard Howitt (2001) argues that the Western techno-centric approach to educating resource managers is deeply flawed. First, because Western resource management purports to be value-free by virtue of its numerical representations of resources. Second, because this claim of objectivity erases its main assumption: that teaching to be "better" resource managers is to produce "more" resources, and more quickly for the market. The supposedly "value-free" sciences of neoliberal natural resource management, Howitt argues, systematically disenfranchises Indigenous peoples through constructing knowledges about "resources," at the same time that it produces ecological problems from overexploitation and overproduction. "The myth of objective and neutral resource management" needs to be abandoned to instead develop a field of environmental management that can teach how to measure success in terms of "social justice, ecological sustainability, economic equity, and cultural diversity" (Howitt 2001: 5, 10). Many have likewise critiqued quantitative representations of science, explaining how they can serve to affirm the expertise of the scientist at the expense of other ways of knowing (St. Martin 2006; Whatmore 2002; Demeritt 2001).

However, these critiques of "Western" science should amount to more than a criticism of quantification. This is also why I have found the theoretical frameworks and methodological practices of sciences studies useful: through their careful ethnographies of scientific practice, these scholars show many ways that "Science" is itself a "local" practice, practices infused with cultural assumptions (Powell 2007; Nader 1996; Galison and Stump 1996). These literatures indicate that the problem is not just the reductionism of quantification per se.

I thereby designed a study whereby I could study knowledge-making for governing practices: the events that make up the collaborations between Indigenous peoples and regulatory biologists; their data-gathering and representational practices; and managers' subsequent debates over this information for decision-making. Through engaging with the literatures from critical geography,

Indigenous Studies, and science studies, I analyze the practices of one co-management regime, that of migratory birds in North America.

Case Study: Sites and Methods

I studied the practices of researching and managing greater white-fronted geese (*Anser albifrons frontalis*), a transnational migratory species that nest in lands occupied by Koyukon Athabascans in Alaska. Because these geese migrate from Alaska to Mexico, hunting is regulated following guidelines established by the International Migratory Bird Treaty of 1916 (MBT 1916; USFWS 2002).[5] Thus, the responsibility for monitoring migratory populations as they move through the United States falls to the USFWS. But by 1998, when Alaska Natives re-negotiated the treaty to legalize their traditional subsistence practices, the USFWS facilitated the design of a co-management structure called the AMBCC, where since 2000 a USFWS representative as well as that of the Alaska Department of Fish and Game (ADF&G) sit alongside ten Alaska Native representatives to decide upon the subsistence regulations for harvest. In this article, I examine data sources and information of this regulatory regime: what observations and analyses, and subsequent discussions during decision-making, constitute regulatory knowledge.

I completed a "multi-sited ethnography" between 2003 and 2008, literally studying events taking place at different locations (Watson and Till 2009; Hannerz 2003; Marcus 1995). While my primary field locations were within the Koyukuk and Innoko National Wildlife Refuges (NWR), collaborating with the Huslia band of Koyukon Athabascans to understand IK[6], and conducting field studies alongside wildlife biologists (see Fig. 1). In addition, I observed management meetings where decision-making took place, thus also physically traveling to different Alaskan cities and as far as Washington, DC.

Participant-observation data for this article is from three major visits to the Koyukon communities of Huslia and Galena: of 3 months, 7 months, and 4 months, between 2003 and 2005. During these trips, I observed not just the work of wildlife biologists, but also residents practicing subsistence fishing and hunting. I also made shorter trips of up to a week to observe regulatory monitoring practices and wildlife meetings, including observing a 1.5 year cycle of AMBCC's regulatory meetings, from 2004 to 2005, reviewing and coding transcripts of meetings from 2004 to 2010 to reveal patterns in the data (Watson and Till 2009). Supplementing these, I conducted an additional 17 semi-structured interviews of participants in the management process for migratory birds: hunters and Indigenous representatives, academic wildlife biologists and ecologists, and agency staff from the USFWS and the ADF&G; some of these agency employees were also Native.

From 2005 to the present I continue to return to these Koyukon villages once or twice a year, spending all my summers and most Decembers there; it has been essential during the analysis and writing process of multiple publications to check interpretations with key collaborators. Often such conversations led to more nuanced understandings between myself and community members; the research-writing process should therefore be understood as cyclical, iterative, rather than linear. These ethnographic methods of participation and power-sharing are explicitly informed by the literature from Indigenous Studies (most foundational is Deloria 1969). The research process was purposely iterative to avoid studying this Koyukon community as an object of knowledge, a practice that Tuhiwai Smith (1999) argued made "research" a "dirty word" within Indigenous communities. Changing research praxis in this way is not a process of accepting that anything said by Indigenous peoples is "true," nor am I describing a process of censorship by the tribes. Instead, my methods aimed to treat tribal members as research partners, exhibiting respect to Indigenous notions of expertise. I recognized elders, active political leaders, and the Tribal Councils I worked with as legitimate spokespersons for their respective tribes and for representing IK, and therefore these individuals were my primary research collaborators and informants (Kovach 2009; Miraglia 1998). I wanted to understand Koyukon knowledges of wildlife, and understand their experience as "co-managers" in this migratory bird co-management regime.

Finally, to complete my ethnography of the "nature" of co-management, I not only interviewed biologists and managers, but I also read the literature of wildlife biology as a primary source of data, because my intent was to study both practices and representations of regulatory science.

Since I study the practices of co-management, I use narrative to explain those practices in "thick description," an ethnographic technique that also aligns with Indigenous approaches to storytelling (Kovach 2009; Geertz 1973). Thus, in the next section, I present my results as an ethnography, narrating my participant-observations among wildlife biologists. In the subsequent section, I narrate the

[5] The early Audubon society, a social movement composed of ladies against wearing hats with exotic plumage, headed the bird conservation movement that prompted the initial treaty. The series that make up the Migratory Bird Treaty include those made with Great Britain (1916), Mexico (1936), Japan (1972) and Russia (1978).

[6] I do not consider this to be a participatory-action research (PAR) project, but I did negotiate a research topic with tribal members and entered into an official research partnership agreement with the Huslia Tribal Council, offering my services as a grant writer, contributing to and organizing their tribal archive, and other volunteer work in exchange for the privilege of learning from them.

contradictory production of Koyukon IK. I thereby explain how conflicts are produced in the co-management of wildlife, ontologically and epistemologically. In the final section, I discuss what lessons can be learned from these misunderstandings produced in creating regulatory knowledge.

Results

How Humans and Geese "Count": The Scaling of Regulatory Knowledge

I first recount the formation of this regulatory research agenda. These results show that practices of scaling regulatory knowledge are simultaneously field practices and practices of representation for the management regime. These are practices of scaling knowledge of geese, which inadvertently marginalize the perspective of the tribes who are supposed to "co-manage" species.

It was not until the early 1990s that the USFWS identified white-fronted geese as birds of "management concern."[7] Biologists with the local Koyukuk–Nowitna NWR Complex learned of a local narrative of the population trajectory: told by Koyukon elders from the villages lying within refuge lands. The local refuge biologists habitually speak with these elders during their visits to the villages (Bryant 1994). All agency biologists acknowledge Koyukon expertise about local subsistence resources. This respect comes in part because some of these biologists are Natives and grew up in the area, so not only are they related, but culturally trust IK. Yet, the more numerous non-Native biologists too claim great respect for elder knowledge. What I am about to show, however, is how IK can be erased through the scaling practices whereby biologists produce regulatory knowledge of wildlife.

Interestingly, local refuge biologists began recording interviews with many of these elders on their knowledge of wildlife resources. These elders explained how their springtime skies used to be filled with "clouds" of birds; the late Catherine Attla of Huslia said that in late April every year, residents would start hearing birds:

> Even in the village we used to hear a lot of birds—geese flying over, swan, crane—see, I never hear a crane all spring yet, this is one of the first ones that usually come. It used to be like music….Just like music, never let up, all 24 h around, night and day. Never let up….Right now I heard one bird, that's all, I don't even see geese fly over (Attla 1995a).

Her husband Steven Attla gave similar testimony, also noting that the birds are not distributed evenly across the landscape (Attla 1995b). This decline was a dire situation to the elders, as late-spring food security along the Koyukuk depends upon migratory waterfowl. Just when the moose meat becomes depleted (or too "freezer-burned" in the era of modern technology), the "speckled-bellies"—the Koyukon name for white-fronted geese—are the first migrants to arrive on the lakes and rivers still choked with melting ice, after a long winter, and just before the greening of the boreal forest. In their conversations with local biologists, elders identified that the "speckled-bellies" seemed to be declining more than other waterfowl.

Yet, biologists involved in the community research had a difficult time convincing other regulatory biologists that these oral histories qualified as valid data. Elder knowledges were "not objective," not produced through "rigorous" methods, not quantified.[8] One local refuge biologist argued to his superiors, located in urban Anchorage and Washington, DC, that these elders had long-term ecological knowledge about the region. But non-local biologists criticized, "well, everyone says that the fish used to be bigger, the women prettier…" Claims that the geese used to be more abundant, their singing "like music" sounded like nostalgic reminiscences.

IK, as both a qualitative and local knowledge, did not fit into the global "unity" of the natural sciences (Galison and Stump 1996; Demeritt 1996). Philosophers of science have long debated the (given) nature of scientific unity. The concept describes both the interconnectedness of natural phenomena, as well as the unity of a single truth of reality, with the same methodological process to articulate the unity of that truth (Galison and Stump 1996). Many wildlife biologists implicitly and explicitly understand the data they produce to lie within such a "unified" practice. As noted by the oft-cited paper on "Gaining Reliable Knowledge" in wildlife biology, the "facts of nature" are assembled like bricks for a home, and thus support the larger theories of nature and natural processes (Romesburg 1981, 293).

Yet interestingly, these practices of regulatory wildlife biology, like IK, also do not fit into the global "unity" of the natural sciences. In recounting this early history of migratory bird research, I have had to designate new subcategories of wildlife biologist: the local field refuge biologist versus non-local regulatory biologist. For a tension arose between the local biologist and his non-local superiors. Not only does this signify a disruption in the

[7] This is in contrast to 'birds of conservation concern' as identified by the USFWS, which may or may not be hunted (USFWS 2002).

[8] Unattributed quotes come from agency personnel, interviews I conducted in 2003 and again 2005 with many of the same people; I have not included much detail about these people other than their quotes for concerns over anonymity. Many of the individuals I interviewed no longer occupy the current staff positions.

unity of the sciences by signifying a "locality" to regulatory scientific practice. This also signifies a political tension: over what constitutes valid knowledge about geese for the purpose of management.

The knowledge produced by the regulatory regime became based upon this question: what is the "population trajectory" of *Anser albifrons frontalis*? To answer this question determined how geese would get counted for management. This question also determined how humans would "count," in terms being witnesses of environmental phenomena, and in terms of how their actions become calculated as affecting that "population trajectory."

To "check" the oral history data about geese, the local refuge biologist received funding from the USFWS to conduct quantitative studies, federal expenditures for surveys that by the early 1990s had largely ceased in all but a few areas deemed critical in Alaska (Saperstein 1996). One was a brooding (or age-ratio) survey that takes place annually in July along the Kaiyuh Slough[9] in the Northern Innoko NWR. I participated in this survey in 2005, and found that its methods aimed to affirm the unity of the sciences—and inadvertently results in the exclusion of IK and local knowledges in wildlife management.

Paradoxically, this exclusion comes because the sciences are themselves not homogeneous: the more different, the harder they strive to affirm their unity. Scholars have recounted critiques of field biology and ecology as mere "natural history" (Kohler 2002; O'Connor 2000). Ecology itself has been branded "a 'subversive science,'" founded within the context of the 1960s environmental movement (Kinchy and Lee Kleinman 2003 869; Shepard and McKinley 1969; Sears 1964). Sustained field work is additionally the target of critique by both expert and lay publics. Research biologists from regional offices or universities critique field biologists for "shoddy" methods, for example their lack of random field site selection, often chosen when tightening government budgets constrain studies to convenience surveying along roadsides (Cohn 2005; Anderson et al. 2003; Anderson 2001). And in regulatory meetings, non-scientists critique biological field work not only because Native and non-native hunters observe environmental phenomena, but field biology lacks the "truth-granting status" of the laboratory (Latour 1999).

Unlike the field, laboratories are the "best" places to achieve certainty; they have the cachet as a "legitimate" place to conduct science because they allow for the easy separation between subjects and objects. Labs are insulated from the natural environment; lab scientists produce "good" science by remaining in full control of the material context within which they conduct their experiments on objects and produce their data (Latour 1999; Latour and Woolgar 1986).

As scholars from science studies argue, to make their data is not just a practice of "de-contextualizing" phenomena through practices of the lab—but it is also a process of creating the identity of a Western knower. Donna Haraway calls this the "god trick": a way of positioning the scientist as a Subject as if personal and professional contexts did not exist, a Subject she also calls the "modest witness" (Haraway 1997, 1988). To understand reality, objects must be observed by this "objective" Subject. The assumption of an "objective" Subject gives "a clear meaning to the truth-value of a statement" (Latour 1999, 148). Furthermore, the "subject–object" dichotomy assumes that Subjects are human, and that objects are the nonhuman: only the human can be rational, can have language, and can be an actor. When Decartes claimed, "I think, therefore I am," he circumscribed the Cartesian logic that ontologically separated "culture" from "nature" (Huntington and Watson 2012, 2008). Only the human can accumulate and evaluate their knowledge of these other nonhuman objects.

Latour (1993) described how scientific practice developed through making this "ontological divide" between nature and society: between humans and culture (including its sciences) on one side, and nature and the nonhuman (its objects) on the other. In this equation, to allow subjects and objects to collude in representing reality would be to commit the sin of representing a subjective account of the world. Because with subjectivity comes values—and "good" science, including good regulatory science, segregates values from the making of "facts" (Jasanoff 1995). These are the predominant Enlightenment-based assumptions that produce Western scientific work. Labs, in particular, become spaces that are conceived to best allow Subjects to transcend their "biasing embodiments" and thus more easily adhere to the subject–object dichotomy (Haraway 1997, 25). As argued by Latour (1993), this is a "modern" way of thinking.

And this modern ontological assumption drives research practices within wildlife biology. Field scientists aim to affirm their unity with other sciences through attempts at extending laboratory conditions to their fields—including practices that avoid "anthropomorphizing" nonhumans (Ingold 2004).

Field biologists at the local refuge designed the Kaiyuh survey to replicate the laboratory, replicate the human–nonhuman ontological divide, and thus attempt to affirm the unity of the sciences. This was all the more important because biologists reported they were already constrained by the habits of the birds: white-fronts are "not colonial nesting birds; they are widely distributed over a large area,

[9] While USFWS documents call it the Kaiyuh River (eg. Fischer 2006), Koyukon speakers note that this is not a river, but a slough—the name does not end in "*-kuk*," meaning "river"—as in Koyu*kuk*.

in low densities." Not able to randomly select sites for study, the biologists float along the Kaiyuh Slough, one of the primary locations where white-fronts brood in the Alaskan Interior. Regulatory biologists count chicks and adults to later calculate "the proportion of young and the family group size during the brood rearing phase." The refuge staff initially developed this particular survey through "trial and error" by the mid-1990s, whereupon they began to record "consistent data."

Following the current of the Kaiyuh as it weaves away and back to the Yukon River, through the flats inside the Northern Innoko Refuge, the survey always begins the second week of July. Such standardization is key in unifying regulatory field science with laboratory sciences. In addition to temporal regularity, observations take place at consistently slow speeds, about 7 miles an hour. Additional differences become addressed through statistical practices of standardization, and observers themselves are standardized, with half of the four person crew returning to staff the survey from year to year. The survey I helped staff included people representing different subject positions, with differing identities: within the four-person staff on the boat, two of us were women, two were men, three of us were white, and one was Koyukon. The Native was a local RIT.

Although such differences in identities are celebrated under the guise of diversity or "community partnership," the practices of producing knowledge for the regulatory regime purposefully erases these differing subjectivities. Because observers, including Native RITs, must reproduce practices that attempt to affirm their "objective" position—they must become the same kind of witness as anyone else on the survey.

Transforming a witness into a "modest witness" during the survey is accomplished also through particular practices of seeing to count birds: especially when field experience requires the interpretation of a "gray area" not often found in "choreographed" lab experiments (Roth and Bowen 1999, 550). Lynch and Law (1999) argue that "technologies of seeing" can separate subjects and objects in field studies. The boat (its constant speed) and our binoculars on the goose survey were two such technologies. Other technologies of seeing include what Lynch and Law (1999) call a "literary language game," requiring "an active consultation of texts as part of the embodied performance of a socially organized activity" (1999, 320). One such text identifies bird species, a text brought onto the boat but not often consulted because observers have them memorized (Fig. 2). Another text, more often referred to, is a laminated "cheat sheet" that classifies goslings or ducklings based on age (Fig. 3). Both texts serve to abstract the birds into objects: not only do species identification sheets isolate waterfowl from their environments,

Fig. 2 Identification of species and phases of ducklings (from Bellrose 1980)

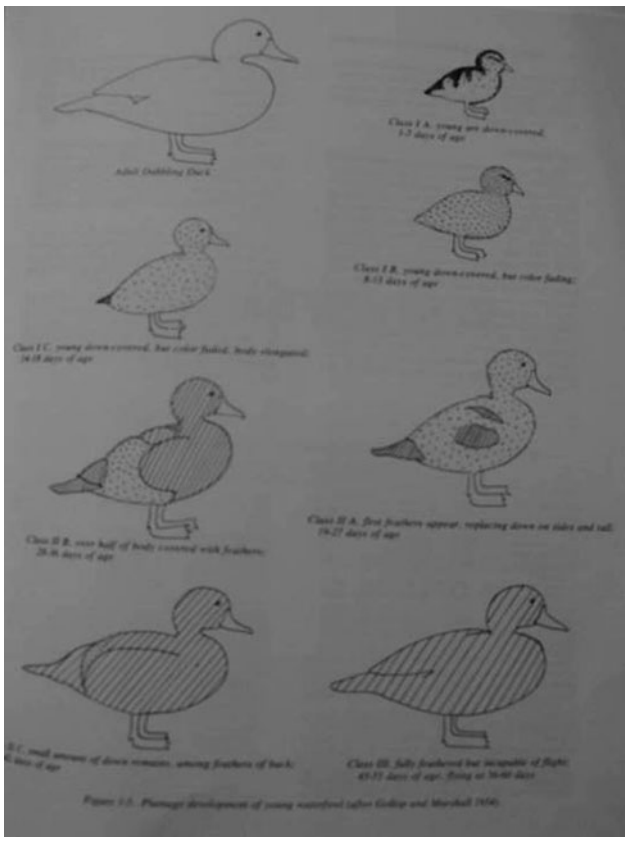

Fig. 3 Identification of species and phases of ducklings (from Bellrose 1980)

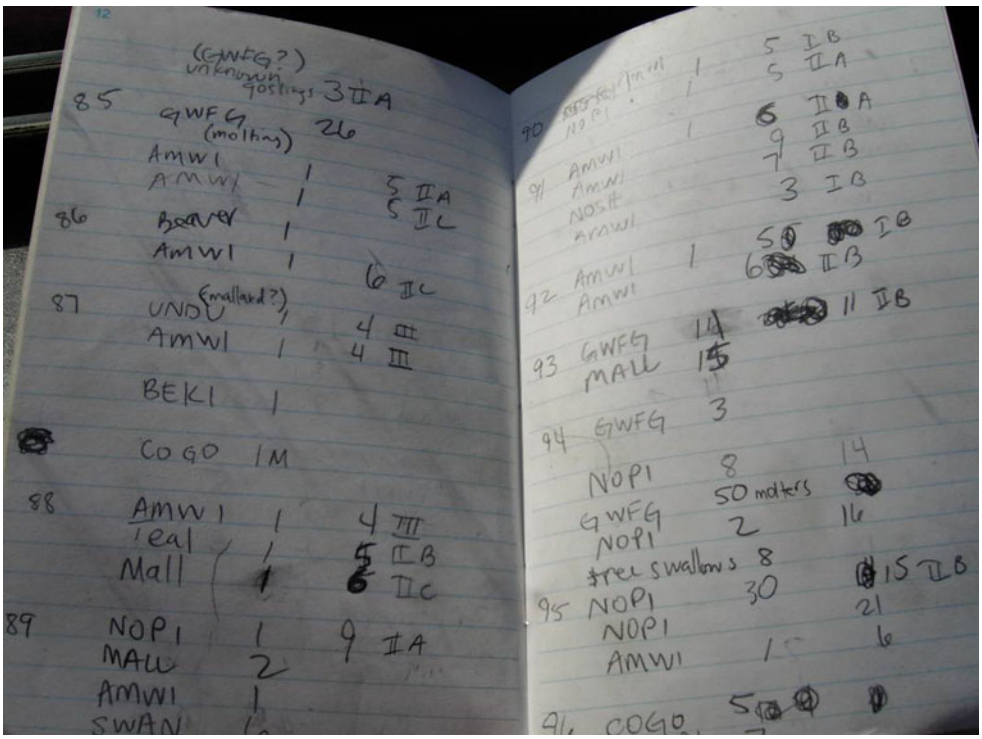

Fig. 4 The logbook used in the field (GWFG stands for greater white-fronted geese)

appearing on uniform and neutrally colored pages, but the age classification sheets further abstract birds into the simplest line drawings.

In addition, scientists do not just have particular practices of seeing, but these are affirmed through having practiced seeing. Some observers trained on a simulation program called "Count," training observers to better estimate large groups of animals, including waterfowl in flight, caribou herds, and salmon runs (Hodges 1993). Wildlife biologists have long worked toward eliminating such "observer bias" that comes from environmental context (Hodges 1993, 96; see also Erwin 1982; and Samuel and Pollock 1981). They define bias as the variability of observers' estimations due to difficult environmental conditions, as well as the animals they miss or duplicate because there are so many to count. Prior to the use of computer simulations, training involved rapidly estimating beans tossed across a table, or the use of photos flashed briefly, whereupon trainees' estimation skills could be rated against the "real" number (Hodges 1993). In addition, there are long-established procedures to estimate a statistical "correction factor," or "visibility index" to account for animals missed in the calculation of censuses[10] (Pollock and Kendall 1987; Bellrose 1980).

[10] Including using a 3-year running average for the population trajectory of white-fronts, used in the *Mid-Continent Greater White-Fronted Geese Management Plan* (USFWS 2005b).

The USFWS employees, trained how to see an individual animal in a landscape of visual "noise," focus on only pieces of waterfowl: the eye bands, the profile, or the wing bars—which are then "scaled up" as individual animals for recording observations. Once observers identify a bird, and when another observer confirms the first observation, one person hand-records that sequentially numbered data point onto a map, while another records the observation into a logbook (Fig. 4).

Figure 4 of the logbook first shows how information about birds are even further abstracted and decontextualized—the names and age class become coded, and made to stand for the reality of the observation. Haraway observed that "[s]truggles over what will count as rational accounts of the world are struggles over how to see" (Haraway 1988, 587). All of these techniques are designed to standardize the observations of a Cartesian, modest Subject; individuals in the boat on a survey might change, and they might in other situations act through their differing subject positions, but these scaled techniques of seeing remain the same. And so observers' identities simultaneously become equalized.

However, Fig. 4 also shows the literal "stammering" of fieldwork (Latour 1999)—the erasures and crossing-out of numbers demonstrate the moments of confusion over the observations themselves. This "stammering" came from our environmental context: for example, the summertime fire regime that characterizes boreal forest ecologies. At

Fig. 5 Landscape of the Kaiyuh flats

one point on our journey, smoke began to cloud our view of waterfowl. Although not extreme this particular year, one of the observers said that during other years of the survey, the smoke from a fire hugged the boat so close as to obscure much of the landscape. In addition, discerning species and their number was difficult because we traveled across the Kaiyuh flats. Because permafrost underlies this boreal landscape, and because the slough winds from one boggy part of the Yukon River to another, the survey route wound around lakes rather than a linear "river." The flats made the waterscape more like a maze (Fig. 5). It was easy to get lost—the year I was on the survey we got lost for an hour. We saw birds but would not count them; we would not know where to place the observation on our map (Fig. 6) because the GPS unit had been loaned out to an RIT in another village. My fellow observers explained that "getting lost" was an almost annual occurrence on the survey—1 year, they had gotten so lost that they ran out of gas, and had to plead at a nearby village to open their fuel station on the weekend. In addition, counting birds in the flats required intense concentration, for if one was not paying attention when entering these lakes the flight of the birds could seem like chaos: "A whole mess," one of the observers joked to me. "Just write that we saw a whole mess of birds!"

Despite the techniques that scale up their observations, wildlife biologists cannot control their field as much as a laboratory scientist. The "materiality" of field science confounds an easy separation between subjects and objects. The physical terrain continually affected data collection; yet in representing the data from the Kaiyuh survey, compiled with three additional float surveys in the Western Interior region, authors write they survey a total of 539 "river" miles (Marks and Fischer 2012; Fischer 2006; Fischer et al. 2005; Spindler et al. 2001a, b). These texts do not mention circuitous routes or possible effects of wildfires and open lake landscapes; the "unity" of Science is imposed upon a local heterogeneous practice. Regulatory field science operates with strategies of representation in mind, and refuge biologists black-box "biasing embodiments" through procedures and techniques that demonstrate their unity with other Sciences.

The structure of management that emerged to modestly "know" these geese is important to recount. Like many migratory birds of North America, white-fronts migrate from Mexico to Alaska, and the management structure that monitors and regulates them is organized as a Flyway System, instituted in 1951 to echo the migratory pattern of most North American birds. Since the Migratory Bird Management (MBM) Treaty did not initially consider the different seasons of harvest for Alaska Natives, their harvest was illegal until the 2000 renegotiation of the international treaty. This formed the co-management body known as the AMBCC; AMBCC's recommended regulations are forwarded to the Service Regulation Committee (SRC) of the USFWS, composed of representatives of the US Department of the Interior, who then decide whether to pass these regulations as consistent with the Treaty.

From his office in Anchorage one biologist from MBM explained about the local refuge research efforts on white-fronted geese: "We watched those numbers [of birds] go down for a number of years." And the decline prompted biologists to organize this research from within the MBM office in the city of Anchorage. As one of the Anchorage biologists described it, centrally organizing the research "as it should be," initiating a more "coordinated" effort to study geese decline. The MBM office funded other Alaska refuges to conduct aerial molting surveys to assess whether

Fig. 6 The survey map in the field, including within the lake system of the flats

the decline of nesting Alaska white-fronts was a larger-scale phenomenon, and when those studies also indicated a decline, MBM biologists "started asking questions about that particular [age-ratio] survey [on the Kaiyuh], to try to understand what it is we [were] actually counting, whether it's an appropriate method to monitor population size."

For "in the regulations process, molting surveys have not been used as an index before." Because the populations at molting sites can fluctuate as much as 60–70 % in 1 year; birds might shift their nesting patterns, or not nest in one particular year. Or there might be many non-breeders, who without young could travel to more distant sites in Alaska, where there is better forage to support their nutritional needs for rapid re-growth of feathers during the molt. To regulatory biologists, it "just doesn't make sense" when all the birds, gone 1 year, seem to return the next.

That is, it "doesn't make sense" if you: (1) prioritize an accounting of the "overall numbers of abundance"—the entire population of white-fronts; and (2) want to see a smooth linear progression of "the population" for the purpose of regulating human harvest year-to-year. MBM biologists questioned the purpose of continuing nesting counts to represent the entire population. They asked: "were they a reflection of abundance, or were they a reflection of changing breeding conditions?" They concluded that the molting surveys, "focused at very specific locations in Interior Alaska…might not be the best estimate for how a population is doing as a whole."

In explaining this, the MBM biologist addressed the oral history evidence: "Now that might not be all that meaningful for subsistence hunters that normally go out to a molting site and hunt birds, because that's a very small scale area, where hunters from Huslia or Galena or wherever go to a specific site every year, and for whatever reason there's fewer birds there." In that case, he said, of course "you're going to be concerned." The problem with that data from Native witnesses, as he and other regulatory biologists saw it, was this small scale. As another interviewee noted, "What managers need is a number they can understand exactly what that number means."

Thus, "making sense" of the data for many interviewees positioned within agencies meant that data must be legible to managers positioned within the flyway structure. It is because of this audience that biologists wanted to author a population trajectory as a smooth linear progression and as representative of a "whole" population. Crucial is the scale of the data in this representation: "management for white-fronts is over a very vast region of breeding: they breed in

Canada, they breed in Alaska, and [managers were]… trying to figure out what the population as a whole was doing, and not so much a fine scale of changes." Although the local refuge biologists pursued a number of quantification strategies, their surveys could not represent the whole population for the purpose of international regulatory management of hunters. Therefore, the representational demands of management—the flyway structure itself—determines what data would "count." The management regime privileges aspatial calculations of abundance over spatial patterns for their management goals.

The surveys that non-refuge biologists today use in their reports to AMBCC are those conducted via aerial methods outside of the United States. Because, having tracked their migration pattern via telemetry studies, biologists found not only that white-fronts have two separate migratory patterns, the Pacific Flyway and the Central Flyway, but also that most of the Central or mid-continent population of white-fronts stage during fall migrations in the prairies of Alberta and Saskatchewan. This enables biologists to conduct a single survey to count their entire population at once (Ely and Dzubin 1994). So while other places, census activities, and researches are still part of biologists' and managers' assessment of population health—like the Kaiyuh survey—they become "ancillary" information for managers. The Canada-based fall aerial survey determines how these birds "count" for the *Waterfowl Status Report* (USFWS 2006; USFWS 2005a) and for the *Management Plan for Midcontinent Greater White-fronted Geese* (USFWS 2005b). This Canadian count, calculated for a 3-year running average to further smooth the population trajectory, determines the population thresholds to trigger particular management options that either support or restrict hunting in Koyukon lands.

As a result of these research and representational practices, leaders at actual co-management meetings hardly discuss "speckled-bellied" geese. The research enabled more restrictive regulations upon hunters in Texas, who biologists found were more successful hunting white-fronts than other species. And managers report today that the population of white-fronts have stabilized (Marks and Fischer 2012). However, because the way the data on white-fronts is scaled, some species become more visible to AMBCC than others. AMBCC is unable to address their differing spatial patterns in decision-making; it is as if the species does not exist. White-fronts as a whole species are not as "threatened" a species as others that nest in the Alaskan Interior and Arctic—even though tribal members from multiple Koyukon villages still decry their decline.

Other scholars have noted that the scale of observations and practices by local knowledge holders often do not "match" the scale of management, leading to disenfranchising local peoples (Meek 2010; Adger et al. 2006; St. Martin 2006). But I am saying more here: that the represented scale of regulatory knowledge stems from ontological and epistemological commitments of Enlightenment humanism. The data is being scaled globally for the purpose of devising hunting regulations at the local scale—"nature" in one sphere being saved from the activities of "culture." Significantly, these scaling practices of regulatory wildlife biology also erase very different kinds of human identities that comprise the IK of non-humans. They also therefore erase different kinds of data that might be produced for co-management.

Koyukon Knowledges of Geese, and the IK of Human–Nonhuman Relationships

In this section, I report the results of my participant-observations with Koyukon communities. Koyukon IK (as one of many kinds of Indigenous ways of knowing) proceeds from its distinctly different ontologies and epistemologies in comparison to "universalized" biological knowledge. I describe here how this Koyukon IK is misunderstood because of their different ontological and epistemological assumptions.

First, the IK of Koyukon or other tribes is not merely "local," that is, it is not merely knowledge with a local spatial and temporal purview. Rather, IK is a spiritual knowledge, what Berkes (1999) called a "sacred ecology." IK is an ethical knowledge whose ethics derive from its non-modern ontology, one that refuses to separate nature and society (Watson and Huntington 2008). This is the "spirituality" regarding the animal world that is sometimes referred to within Indigenous Studies as "Native Science" (Cajete 2000). As such, IK is not merely "local" but ethical.

Significantly, Koyukon see nonhuman animals as ontologically the same being as a human—and would not think of calling one an "it" as do scientists in talking about the objects they study (Huntington and Watson 2012; Ingold 2004; Salmón 2000). In other ethnographic accounts, I co-wrote with a hunter (Watson and Huntington 2008), we described the ways in which the human–nonhuman relationship expressed a "relational ontology": nonhumans are kin to humans, and these nonhumans choose to participate in hunting when they choose to "give themselves" to a respectful hunter. A respectful hunter not only respects the individual animal by killing quickly, but also by taking care of the landscape that had fed that animal throughout their life. This knowledge, including its ethical obligations, is shared while also sharing the nourishment of the animal's body with the human community at large.

These ontological commitments amounts to "anthropomorphizing" non-humans, according to the discipline of wildlife biology. However, they lead to very different

approaches to both the production of knowledge and the production of "management." Latour (1993) suggested that such "non-modern" peoples would exhibit a different kind of environmental ethics. Such difference is most apparent in the Koyukon critique of scientists' treatment of birds.

For example, many Koyukon exclaimed how the local field biologists "bothered" animals, referring to biologists' research practices such as neck banding. One elder exclaimed, making a choking motion around her own neck, "How would you like it if you had" to have that "around *your* neck?" [11] Many community members do not approve of "invasive" biological techniques used to monitor species, whether it be neck bands or implants of tags in salmon. A few tribal members recounted the summer that biologists "shot rockets" into the sky to catch birds in their nets for tagging—a procedure required for the telemetry study that precipitated the "discovery" of the geese's migratory path. In my interviews with them, biologists too recount the strong community resistance to their techniques—while they claimed to respect Natives' beliefs, they explained that despite the "drawbacks" of using such techniques, there are "many advances" these techniques bring to their knowledge of birds. And as noted, understanding the migratory path led to targeted regulations that stabilized that species.

Koyukon, however, argue that "bothering" animals causes their decline. One elder said, "they shot nets over their heads to do their banding, but you can't bother them like that. Those ducks remember. And so they never came back to that lake." Many also claim that neck bands kill off the birds who have a harder time eating. Some of the elders' most emotional stories expressed their horror at this treatment of animals. Scientific practice, they argue, negatively impacts conservation—many argue that if scientists treated the birds with respect, they would not "bother" them in this way. Koyukon ontology—understanding humans and nonhumans as the same—leads them to this particular ethics. Ironically, recent biological studies show that techniques such as banding increases bird mortality—human hunters and other predators could detect their prey more easily, in addition to some birds dying after difficulty eating (Alisauskas and Lindberg 2002).

Koyukon ontology leads to other disapprovals of scientific practice. For example, one of my tribal collaborators, who knew that the brooding survey crew would get lost every year in the Kaiyuh Flats, said he knew that the people who subsisted in the area could not be so debilitated by its slow-moving water. He advocated hiring a local elder to show biologists the "right places," and where the geese hid in the grasses. Designing the Kaiyuh Survey, the local biologists hired a local elder—but stabilized his route by mapping with a GPS to maintain "unity" with the sciences.

Another of my Indigenous collaborators, also a wildlife biologist, argued that the route's (relatively consistent) repeatability actually limited its explanatory power. For he recounted that, as a biologist, he could not employ the instincts that he developed to think like a goose—thinking about, if he was a goose, where would he live? Such "subjective" and irregular methods were outside the bounds of the discipline of wildlife biology, because they did not adhere to the separation of subjects and objects. The survey route—now committed to the map—would not allow surveyors to adapt to where the geese might be as they responded to the local context during a given spring season. By contrast, my informant thought that biologists should be like hunters and follow the birds, not the route. Yet, as a biologist—performing his duties as a "modest witness"—he could not suggest to the survey crews to check the higher banks, where he said he had felt the presence of geese. Biological research indicates that geese "preferred" the floodplains—an expression of statistics—while in the same study the biologists noted that a full third of the nests were in "uplands not subject to flooding" (Spindler and Martin 2001). Because the survey crew relies on a model of expertise whereby all experts are "modest witnesses," the Native biologist argues the survey misses an important opportunity to produce a better accounting of local birds, and not just a bird count.

These results show how Natives' lack of political power comes not from whether they are positioned within the "democratic" regulatory decision-making structure. For indeed, Alaska Natives participate as "co-managers" in the AMBCC structure, and the USFWS employs Native RITs and Native biologists. This is where questions of "identity" become important. Harvest data might be more reliable because a fellow community member is collecting the data, but the methods of wildlife biology dictate that the identities of the observers cannot count in the making of biological knowledge. This is why the Native AMBCC representative claimed that Native RITs do not represent the communities—they are not elected, and they do not represent IK because they do not use those methods in their jobs. The data produced through and for the regulatory regime is only constructed from Enlightenment assumptions that separate humans and nature, regardless of the race/ethnicity or the beliefs of the person performing those data-gathering tasks.

[11] Many Koyukon told me of their disapproval of "bothering" animals because they positioned me as working for "game management;" I constantly identified myself as "a university researcher, I don't work for fish and game." But as my interest was in wildlife management, many desired to teach me what they felt was the correct way to treat animals.

Natives' lack of political power comes not from whether they are at the management table, but from whether their different ontological and epistemological assumptions about nature and society produce information that is legible to the management regime. Thus, my account shows how Indigenous ways of knowing are already marginalized even before their relevance can be discussed at the management table.

Conclusion: Misunderstanding the "Nature" of Western-Indigenous Collaborations

Co-management has been touted as an alternative to "command and control" approaches to natural resources, and collaborations with local indigenous communities have been seen as a path to create more "resilient" and "adaptive" ecosystems. Yet, by examining the power dynamics inherent in the practices of making regulatory knowledge for the AMBCC co-management regime, I show that the regulatory regime misunderstands and thus maligns the "nature" of these supposedly equal collaborations between scientists, managers, and tribes.

I showed how each participant in regulatory management observes the same environmental reality, but the subsequent methods and measurements that are privileged to represent environmental phenomena produce different kinds of data—some of which are rejected for consideration in the regulatory regime. I show how the co-management of white-fronted geese does not just slight a "local" spatial perspective. The Indigenous communities and the regulatory scientists I worked with operate from different ontological and epistemological assumptions of nature–society relations. While Enlightenment-based practices in wildlife biology avoid "anthropocentrism," Indigenous Studies describes identities that operate through non-modern, deeply imbricated human–nonhuman identities that do not separate "nature" and "society" in making knowledge.

"Scaling" the data for the co-management regime might not marginalize Indigenous peoples, but they can marginalize Indigenous understandings of human–environment relations. When managers think that they are getting "community" representation and IK from any Indigenous person participating in the regulatory system, they are misunderstanding the role of Indigenous identities in the making of Indigenous expertise. This is how Native peoples can populate a management decision-making structure, while still be open to the criticism that Native knowledges are continually maligned.

Crucially, misunderstanding the "nature" of their collaborations causes biologists and managers to measure the system in ways that erases how Indigenous groups already "manage" wildlife: by living through their ethical commitments to their fellow beings. The only way that Indigenous people "count" in the management system is whether they report their harvest to managers. Research questions at the heart of regulatory wildlife management do not ask so much about how to conserve birds, but how and when to stop people from hunting them (Marks and Fischer 2012). The very purpose of regulatory research questions aim to determine when to prevent their kinship with these nonhumans, a kinship that defines who they are as beings. These misunderstandings of "nature" have real, political effects—felt in the everyday lives of tribal members who practice subsistence based upon their IK. How can a Koyukon hunter accept the spirit of an animal who chooses to give himself to the hunter, if that animal gives himself the hours before or after a regulatory window? These misunderstandings are at the heart of many wildlife management controversies erupting at multiple scales (e.g. Watson and Huntington 2008; Wishart 2004).

Demeritt (1996) has explicitly called upon studies of science to be more prescriptive in articulating how to practice a science that is more just. Science studies scholar Shiela Jasanoff argued that "co-production" is not just realized through democratic debate and conscious negotiation of values, as suggested by the scholars of adaptive co-management. For her, "co-production" is practiced; that is, "the ways in which we know and represent the world (both nature and society) are inseparable from the ways in which we choose to live in it" (Jasanoff 2004, 2). She says an account of "co-production" can "take on the normative concerns of political theory and moral philosophy," in an ethical sense (Jasanoff 2004, 4). Co-production on this epistemological level is embroiled in power relations, and thus little understood by the literature on adaptive co-management—yet this lacunae has serious implications to the very success of an adaptive co-management regime. For is there really "learning" in a supposedly "adaptive" system if the practices of representation in science erases the identities and knowledges practiced on the local level? Even though North American white-fronted geese populations stabilized, in the Koyukuk River Valley, food security is still a concern—and isn't the point of co-management to actually benefit peoples at the local scale?

Hence, I argue with science studies scholars that co-management regimes might also more effectively co-produce data. Like Latour (2004), I argue that it is less important to bring science to processes of democracy than it is to "bring democracy to the sciences." Recently, Daniel Wildcat decried that IK would "save the planet" (Wildcat 2010). Environmental managers can learn from these ontological and epistemological differences in this and other cases involving Indigenous peoples; multiple ontologies might be better represented in the work of producing

data for environmental management. Surveys or other research efforts might produce different results if guided by the insights of Indigenous Studies: rather than flatten out the identities of witnesses for the production of survey knowledge, biologists might instead regularly employ elders to go on these surveys every year. Some residents I spoke with suggested that doing so might allow the elders greater understanding of the current spatial dynamics of the birds. This becomes especially important, since elders are less likely to go out into the country the older they get, while they possess the greatest knowledge base of ecological dynamics and ethics. With their new experience on the land, elders can tell stories about the birds to inform hunters how better to relate to these birds—and thus better inform the local communities concerned with their management. In this way, the "adaptive co-management" model might be applied across both communities and institutional structures. Similarly, the USFWS and its refuges might be better served to be staffed by elders or Indigenous Knowledge-holders. Regulatory scientists might also be better trained to be interdisciplinary, and trained cross-culturally. There are few positions in the agencies deeply involved with AMBCC that employ social scientists, for example—except as experts of "culture," often anthropologists who employ various quantitative techniques to calculate "use" (including trade) of the species—again, involved in research questions that aim to calculate how to separate Koyukon "culture" from the "nature" that constitutes part of their identities.

Furthermore, it is important to remind managers that Koyukon IK (and IK of any tribe) had functioned as already-governing wildlife management long before the regulatory regime emerged. Regulatory wildlife biology disenfranchises knowers who understand the geography of the world differently, who have "wisdom." Biologists can supplant Indigenous peoples as legitimate knowers of wildlife even while they claim they are "partnering" or "collaborating" with tribes.

Significantly, this also means that these ways of knowing do not have to be Indigenous alone. As stated by Latour (1993)—*we* (meaning Western, Enlightenment-based scholars) have never been modern. We only think (and act, and represent) that we are. Western academics in our varying disciplines might learn from IK.

In this article, I am not arguing to completely abandoning Enlightenment-based concepts of expertise and data formation; I am not arguing for the eradication of the RIT program, or that wildlife biologists stop their global accounting of species. I am not arguing here that the methods used by wildlife biology should be abandoned; I am not arguing that "objective" research is "bad" and "subjective" good, because I have shown that this dichotomy does not exist in the making of knowledge. Not at all. I am arguing to expand our ideas about what counts as knowledge, and what makes one a human. Wildlife co-management might begin to address research questions that are founded upon different assumptions of nature and society. Goldman (2007), for example, gives an excellent example of how to utilize both the methods of ecology (such as using GPS units for locating distances) and the Maasai techniques of measuring through time. The question my study leaves me with is: what if wildlife co-management systems privileged research questions other than species abundance? Can working with IK prompt more spatially concerned research questions? What else could IK tell managers about the system if their data were more visible to the regime?

To develop methodological practices that accord equal power to differing peoples and knowledge systems, this requires that both Western and Indigenous knowledge systems be equal in the "co-production" of regulatory data. This would bring greater democracy to the sciences—instead of bringing science to democracy. Dealing in these ways with power relationships might lead to learning about the social–ecological system in ways that adaptive co-management initiatives so far only hope.

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